STUDIES IN THE PSYCHOLOGY AND PHYSIOLOGY OF LEARNING.[1]

By Edgar James Swift.

I. On Tossing and Catching Balls.
II. On Learning Short-Hand.
III. On the Acquisition and Control of the Reflex
      Wink.

Learning, though as various as human activity, may be roughly classed under one or the other of three types: The acquisition of skill, or learning to do; The formation of associations, or the acquisition of information; and the getting of control, or the formation of inhibitions. While it is doubtless true that it would be difficult, if not impossible, to find absolutely pure types of any of these, so fully is mental action of all sorts involved in each, yet for purposes of preliminary study the classification is adequate and each type is considered in the studies that follow.

I. On Tossing and Catching Balls.

The purpose of this study was to investigate the learning process, to inquire whether there is a typical curve of the acquisition of skill, and if so to determine its general form and so far as possible to find out what alters it in particular cases.

Method, apparatus and subjects. Keeping two balls going with one hand, receiving and throwing one while the other is in the air, was selected. There were several reasons for deciding upon this, some of which can be best appreciated by those who have tried to do it, but the chief argument in its favor was the accuracy that it permits in measuring the learner's progress.

The balls used were of solid rubber and weighed 122 6/10 and 130 2/10 grammes. This difference was not noticed by the subjects. Their diameters were 42 and 44 millimetres, respectively. Six subjects in all were tested; five with the regular series and one in keeping three balls up with two

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[1] The writer wishes to express his obligation to Dr. Edmund C. Sanford for his generous co-operation throughout this investigation. The President and other members of the faculty of Clark University have also shown interest, and the assistance of the librarian, Mr. Louis N. Wilson, in securing literature has been invaluable.

hands. Five of the subjects were university students and one was a professor.

The daily programme consisted of ten trials, the subject in each case continuing the throwing until he failed to catch one or both of the balls. The number of catches made in each trial was immediately recorded with any data obtainable as to the method pursued and the cause of failure. After each trial the subject rested as long as seemed necessary and then recommenced the throwing until the ten trials had been completed. There was no practice whatever between the tests and none of the subjects had ever handled balls in this way except as the base ball and tennis player may occasionally throw a ball or two into the air and catch them as they come down. All the subjects did the work in the afternoon. In the few instances in which a change of hour was necessary this fact was recorded. The total time occupied in the testing (and this testing constituted also the sole training of the subject) was various in amount extending from a few minutes in the early stages of practice to two or three hours toward the end. All the subjects knew their daily score and they always kept track of their progress during each test as well as from day to day. This method has undoubtedly given results different from those that would have been gotten had the subjects been kept in ignorance of the score, but the plan was uniform throughout and had the advantage of largely overcoming the effect of monotony which usually depresses those who are obliged to practice continuously for so many days. Besides, it enabled observation to be made, incidentally, on the effect of competition both with one's own record and with that of others.

The throwing and catching was chiefly with the right hand in each case (all the subjects were right-handed) but in order to test the effect of right-handed practice upon the skill of the left hand a preliminary test was made upon each subject, on the first day of his service, of his untutored skill with the left hand. This preliminary test consisted of ten trials as usual; and after it the left hand was not again tried until after the completion of the whole period of work with the right hand, when the left hand was again tested and a record of its progress kept for a number of days.

The daily training was continued in the case of four of the six subjects until their average number of catches per trial exceeded 100, or, what amounts to the same things, 1,000 catches in ten trials, upon two days in succession. In the case of the other two subjects the training was broken off at a lower score for reasons that will appear later.[1] The tests with the right

[1] That even the lesser skill attained by these two was not bad seems evident when we find Hopkins saying that "the young man who can

hand were made every day, including Sundays, as were also those with the left, except as indicated below.

Still another phase of the experiment was an attempt to find how skill in the management of the balls declined after the cessation of daily practice. With this in view monthly tests have been made since the close of the regular experiments with the right hand, upon three of the subjects. The results of these "forgetting" tests will be given in their proper order below.

Influences that affected the score. It seems probable that the weight of the balls may have had an influence on the results on account of fatigue, and tennis balls would perhaps have sent the score up faster toward the end when the number of successive catches per trial at times exceeded two hundred. The essential course of the curve of progress would not, however, have been altered.

At the beginning the height of the room proved to be an important element, and the one in which the experiments were made was high enough to allow sufficient freedom in this particular.

The ball tossing proved itself an unexpectedly delicate index of bodily and other conditions. Slight changes in physical condition or in the temperature and illumination of the room often produced noticeable effects upon the score. This results in marked unevenness in the record from day to day, but does not influence unfavorably the general features of progress in which we are here most interested. Indeed, some of these disturbing elements are present at intervals in every learning process, and the importance of taking daily records of the progress in such work, instead of week by week, is apparent when we notice the great variations in skill through which most of the subjects passed.

The most frequent evidences of lack of skill in the earlier days of training were "wild throwing" (the tossing of the ball in such a way that it fell out of easy reach), and clumsy catching, *i. e.,* not being able to capture the ball when it touched the hand. As the subjects progressed somewhat, another source of failure appeared in the collision of the balls in the air, the ascending ball striking the descending one and knocking it out of reach. In the final stages trouble of this sort was again less frequent.

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perform this operation twenty times without dropping one of the balls can treat the artist of the circus as a *confrère.*" (Hopkins: Magic, p. 140.) Hopkins possibly means, however, a young man who *never* fails to reach at least twenty catches, however often he may try. If this is the case, he is speaking of a degree of skill which was hardly reached by my subjects.

CURVE A.
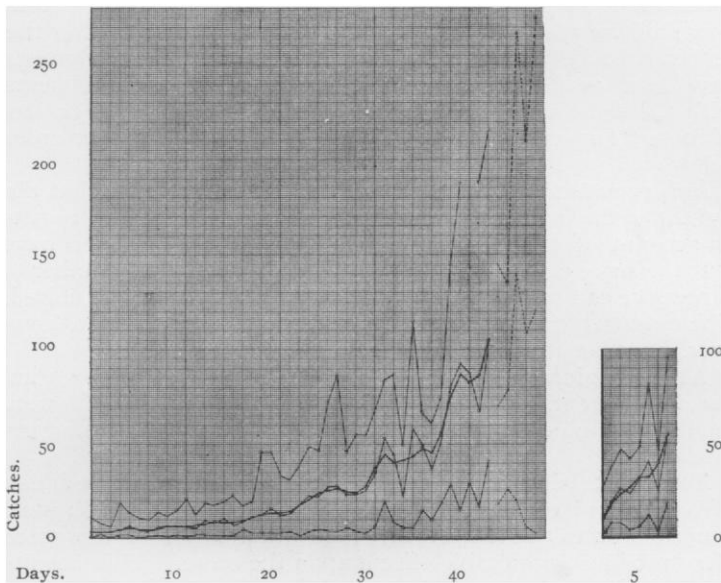
For all the Curves. The curve for the right hand is at the left. The upper, thin, line shows the highest single score (*i. e.*, the greatest number of catches made in a single trial before missing) for each day, while the lower, thin, line gives the opposite extreme, *i. e.*, the lowest single score for corresponding days. The thin line in the middle represents the daily average, and the heavy line that cuts across it is the smoothed average. The method of smoothing has been described above. (See page 210, foot note.)

The number of catches before missing is shown on the vertical axis and the days on the horizontal axis.

The "forgetting" curve is given in dotted lines. The three lines stand for the highest, average, and lowest respectively. Here the unit on the horizontal axis is thirty days. For the discussion of the results see page 221.

The curve for the left hand is at the right. The short line at the right of the vertical axis near its base and perpendicular to it shows the score made by the left hand before the right hand practice began. (See pp. 218-220.)

For Curve A. The break in the highest score, on the third day before the last, was caused by the subject being "called" every time at one hundred. He was trying to make ten scores of one hundred each.

Subject A played base ball when a boy but was only a mediocre player. He had never tried to keep two balls in the air until these experiments were begun. His method was to keep the balls in two parallel columns in front of him. He found himself doing this and then consciously continued it.

CURVE B.
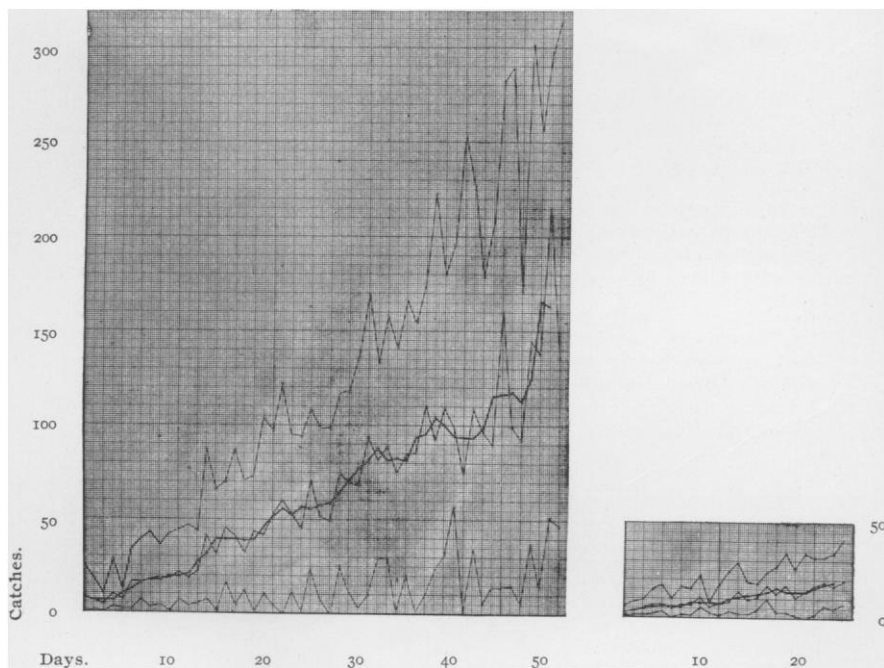
For the general features of the curve see curve A under the heading "For all the Curves."

B played baseball when a boy and has since played tennis.

His steady rise without sudden jumps until the end is probably due to his effort to avoid consciously adopting a method of procedure. (See p. 215.)

His unusually high score on the next to the last day of his regular right hand practice was made during the April holidays, and on that day he was refreshed by a long walk into the country. The drop on the following (and last) day was accounted for by fatigue caused by working at his desk all the morning. (See p. 212.)

B did not take the monthly tests for determining the effect of intermission of practice.

CURVE C.
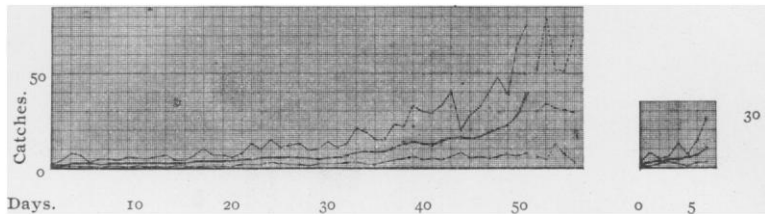
For the general features of the curve see curve A under the heading "For all the Curves."

C never played baseball when a boy and he began his practice in ball-tossing by catching with his hands high in the air and the palms turned forward. His progress was very slow and on the thirty-third day he had evidently reached his limit by this method, the balls gliding down his hand before he could seize them. On the thirty-third day he consciously adopted a new method, holding his hand outstretched with the palm turned up. From this time he made a new start and his progress, though gradual, was steady. (See p. 214.)

CURVE D.

For the general features of the curve see Curve A under the heading "For all the Curves."

D was disturbed so frequently by sickness in his family that his curve is exceptional and shows the effect of loss of sleep on the learning process. He made regular progress during the first seven days and felt, as he said, that he had "the knack" when the effect of loss of sleep for several nights became apparent and he seemed to lose what he had gained. (See pp. 212 and 216-219.)

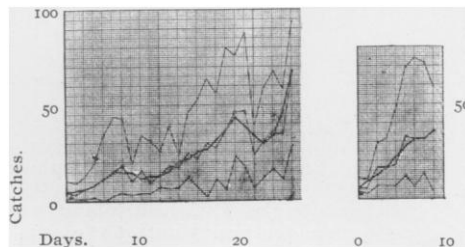D did not take the monthly tests for determining the effect of intermission of practice.

CURVE E.
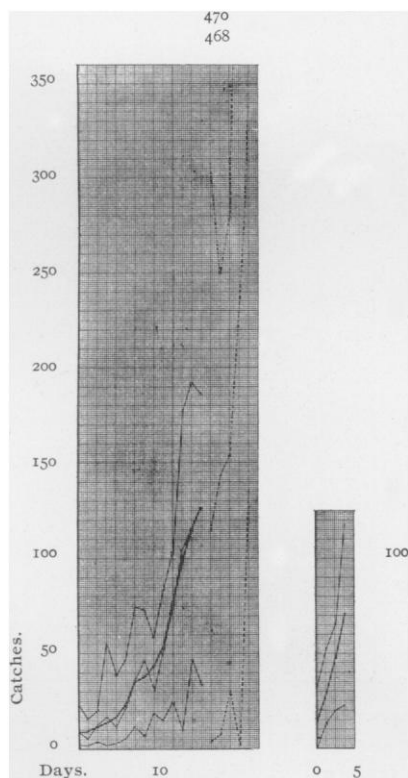
For the general features of the curve, see Curve A, under the heading "For all the Curves."

E played baseball when a boy. In the tests he followed the plan of keeping the balls in two parallel columns in front of him. During the regular right-hand practice his scores were made with a great expenditure of energy, but after the regular practice ended and the monthly tests for determining the effect of intermission of training were made, his increasing skill became evident in the growing ease and grace with which he handled the balls. His highest throws on the last two monthly tests (the fourth and fifth month) were 468 and 470, while his average for these two trials was 229 and 337 respectively. Both of these averages, it will be observed, are far above his highest throw during the regular practice. During the last two "forgetting" months he took more or less exercise upon the tennis court. (See pp. 221-222.)

CURVE F.
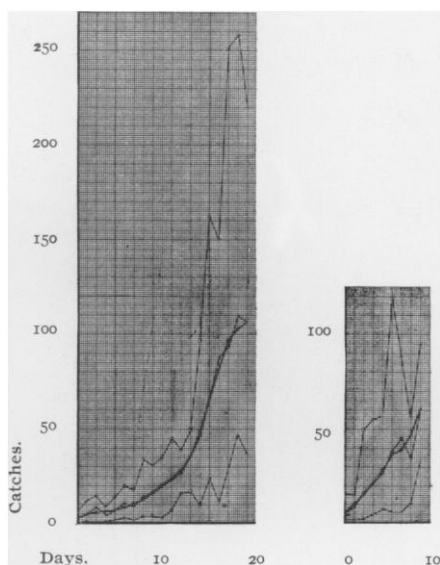
For the general feature of the curve see Curve A under the heading "For all the Curves."

On the third or fourth day of his regular right-hand practice F found himself tossing the balls up at nearly arms-length to the right and in such a way that they took a circular course and came down in front of him. The early adoption of a good method probably accounts in a large part for the uniform rise of his curve. (See p. 215.)

F played baseball when a boy but is not especially athletic.

F did not make the monthly tests of "forgetting."

CURVE G.
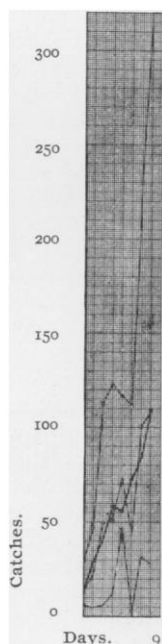
G is the curve for learning to keep three balls in the air with two hands.   The subject had practiced handling two balls with one hand on the tennis court, but had never tried three.

For the general features of the curve, see Curve A under the heading "For all the Curves."

On the sixth day G dropped from 730 catches in ten trials to 431. When he began the day's practice he thought that he was in good condition and confidently believed that he would make his first thousand, but he found himself unable to control his muscles. What had been easy the day before now required the greatest effort and at the close of the practice he was in an uncontrollable tremble. (See pp. 212-213.)

On the last day G made over a thousand catches in seven trials and then stopped.   The cross on the ordinate for that day indicates what would have been his average at the same rate for ten trials.

G did not take the tests to determine the effect of intermission of practice.

None of the subjects gave any preliminary thought to the manner in which they might best get the knack of handling the balls, and this led to individual peculiarities of method, and ways of avoiding or meeting the common difficulties. One subject found himself very early in his practice avoiding collisions by tossing the balls up a little to his right and in such a way that they would take a circular course coming down in front of him. This method was most successful in avoiding collisions. Two others fell into the way of tossing the balls up at nearly arm's length in front so that they took a circular course toward the subject coming down closer to his body. The objection to this method was that the balls tended to fall too near the catcher and so constantly crowded him back. The other two kept the balls in parallel columns a foot or so apart and a little to the right of the median line of the body. With this method the balls got into a "mix up" periodically and then the subjects were obliged to toss high until they could straighten out the tangle when they would settle down again into the same parallel columns, but only to have the experience of trouble repeated in course of time. The same was also true in a measure of the circular throws. We shall return to these personal differences later in discussing the individual features of the learning curves. For other matters bearing on influences that affect the score see the section below headed "Some Conditions that Influence the Learning Process."

In the execution the eyes and attention were upon the balls in the air, indeed, upon them in the upper half of their course. Both the tossing and the catching were executed by the hand alone, for the most part practically outside the field of vision and of attention.

Results. While full numerical data are at hand, it has seemed to the writer that the essential features of the results could be made more easily intelligible by diagrams than by tables, and he has accordingly plotted the accompanying curves. All the curves are plotted in the same way and upon the same scale. Vertical distances show the number of balls caught; horizontal distances the successive days. The main curves show the progress of the right hand, while the dotted portion, immediately to the right of them, shows the result of the "forgetting tests." The lower curves at the extreme right present the progress of the left hand. Points on the uppermost (thin) line show the highest score reached in any single trial on the day in question. The lowest line of all shows, in the same way, the lowest single score from day to day. The thin line in the middle gives the daily average. The heavy line gives a smoothed average.[1]

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[1] The method used in smoothing was to average the averages for

Discussion of the Results. The curves just presented have certain characteristics in common.

1. With hardly an exception the curves are concave toward the vertical axis, which means, of course, that the progress was first slow and then more rapid. The curves which differ most from this type are those of B. In the case of his right hand curve the final rapid up-shoot is postponed and is not so conspicuous when it does come because of the steady, though gradual, ascent that has preceded it. His left hand curve was probably not carried far enough to reach the stage of rapid progress. It is altogether probable that all of the curves would in the end sweep more rapidly to the right and show a stage of slow progress as the physiological limit of skill in such matters was reached, but none of my subjects approached that limit. Bryan and Harter (6) found in their study of the acquisition of the telegraphic language a learning curve which had the rapid rise at the beginning followed by a period of retardation, and was thus convex to the vertical axis. The difference in form is very probably partly due to the difference in the type of learning involved, though it may also rest upon differences in the method of carrying out the test. This will be further discussed under the section headed "On Learning Short-hand."

2. All the curves show great irregularity of advance. Progress is never uniform but always by jumps. The learner seems to make no gain for several days or even longer, then he takes a leap perhaps to get a good grip and stay or may be to drop back a little. But if he loses his hold it is not for long and he soon makes this higher level his starting-point for new excursions.

A growing feeling of confidence usually preceded a permanent rise. The subjects felt that they were "going to do it."

There are not one or two special periods of delay in progress giving a "plateau" or two in the curve, as Bryan and Harter found in their study, in which successful co-ordinations are made automatic. Instead, there are many, the number varying with different individuals, and automatization is going on during the entire learning process. Miss Shinn found this same irregularity in the attempts of her little neice to learn to walk.

3. The average, holding at first somewhat closely to the lowest throw, is gradually drawn away from it by the growth in skill that reveals its reach in the highest throws. Though

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the first three days, then those of the second, third and fourth days, and, again, those for the third, fourth and fifth, etc., to the end. By this method smoothed values cannot be found for the first and last days and in strictness they should have no place on the smoothed curve, though they are connected with it in these diagrams.

the lowest throw does not rise much above those of preceding days the number of low throws continually decreases. The lowest throws are more frequently the result of accidental conditions than the highest. While the latter is always above the learner's usual ability at a given time, it none the less shows the direction in which he is moving, and its height on any day bears some relation to his rate of progress. That is to say, though the learner will not reach the level of a given highest throw on the following day, he will shortly approximate it and get there permanently very soon. The curve of highest throws may in a sense be regarded as a curve of amateur proficiency while the curve of lowest throws may stand as the curve of professional mastery.

Some conditions that Influence the Learning Process in Ball Tossing. It has already been mentioned that execution is greatly affected by physical condition. It is well known that physical experts of all sorts must keep themselves in condition, else they will drop into the class of inferior men. The influence of this factor was evident in all of the subjects. The exceptionally high score of 2,155 catches in ten trials reached by B on the day before the last was made during the April holidays, and on the morning of that day he had taken a long walk into the country. On the following day when his score dropped to 1,359 he had been working at his desk all the morning and did not feel fresh. Everything required greater effort.

D lost sleep so often after the first four or five days, on account of sickness in his family, that his curve came to represent the effect of physical condition on progress rather than the learning process. When exhausted from loss of sleep he lost the skill that he had gained before.

Sometimes the lowered vitality is not apparent to the individual himself. G, who threw three balls with two hands, made a score of 730 catches on his fifth day and when he began on the sixth felt confident that he would make his first thousand. But, instead of this, he fell to 431. After starting he found that he could not control his muscles. What had been easy the day before was now done only with the greatest effort, and at the conclusion of the day's trial he was in an uncontrollable tremble.

Probably the "off days" that all subjects had belong here. These differed from the days when they were simply unable to reach a high score of the previous day. Sometimes they felt no confidence in their power to do any sort of good work though they could give no reason for the feeling. At times "warming up" freed them from this feeling, but again even

the lower scores required much greater effort, amounting in some cases to an exhausting strain.

The correctness of the curves obviously depends upon the uniformity of the effort put forth by the subjects. It is commonly assumed that the maximum effort is a constant factor for a given individual and that the only thing needed to secure it in a conscientious subject is to interest him in the work and then ask him to do his best. While I have no reason to doubt the conscientious effort of my subjects and the practical uniformity of the average effort of each from week to week, the matter is perhaps of sufficient general importance to the precision of psychological experiments to justify a little discussion of it. Even a direct interest in the results of an investigation aided by the no less effective acquired interest aroused by the professional advantage from a well worked out problem will not enable the experimenter himself to make a steady maximum effort in something that has little mental content.

In the ball-tossing the influence of this element was less noticeable on account of its use of the voluntary muscles and because of the counter-effect of the subjects watching their own progress and of competing with others. But even here unintentional relaxation became evident now and then by comparison with the intense effort put into the work at other times, as for example, in the last half when the score of the first was found lower than the subject had hoped for. Anderson (1) found, too, that in strength tests every man but two "failed to equal his best record when tested apart from the other members of the class," and Johnson (15) observed the same thing in tapping experiments. Yet it is easier to hold ourselves to steady intense effort in feats of muscular skill and strength than in many other things because of our mastery of the voluntary muscles. The effect on the short-hand practice (see the second study of this series) was unmistakable and there can be no doubt that it influenced the curve in spite of every effort to resist.

This lack of energy, due to waning interest, probably has more to do with delaying the learner's progress and making "plateaus" than anything else. One cannot escape a dead level in uninteresting work and after the enthusiasm that novelty stirs has spent itself the interest is dulled and effort slackens. This is a potent cause of the long dreary period of no progress in learning to speak a modern language or to play upon a musical instrument.

But the slow progress is frequently only an apparent one and due to our inability to measure the advance. It is a case in which figures tell only a part of the truth. In the ball-tossing during B's slowest period, when the curve showed little or

no rise, it was evident both to him and to the experimenter that he was still making progress and the proof of it, aside from unmeasurable observation, was his occasional high throws. The curve remained stationary because his imperfect training had not enabled him to meet the chance emergencies that were constantly arising.

But the matter of interest is still more complicated. In ball-tossing, after one has reached a fair degree of proficiency, the first part of each trial is always something of a bore till the fifty mark is passed when it becomes interesting. Later the interest may take another slump, rising again after the score has reached one hundred. At this point the possibility of an unusually high score keeps the subject alert to the end. There was also a plateau in the interest of some of the men after throwing for the first time a hundred in a single trial, for which they had been very keen. They felt that it was impossible to reach the two hundred mark at once, the total thousand was too far off to be alluring, and so the edge of their enthusiasm was turned. Later when the chance to make another record seemed good they became as alert as ever. Indeed, after the satisfaction of having thrown a hundred had subsided, and the work continued for a time without great progress, the first twenty-five took on an acquired interest in the anxiety to get done. This brought greater care.

The whole question of maximum effort is well worth special investigation.

The confidence that follows a successful series of throws proved of considerable value, unless it led to the carelessness of over-confidence. Faith in one's ability to get out of a desperate situation in the tossing increases with success. This leaves the attention imperturbed, and one does not "go to pieces."

A long period of delay often represents the physiological limit with a particular method of tossing and a rise is made only by the introduction of an improved mode of procedure. This was especially noticeable in C who caught at first with the hand high in the air and the palm forward and almost perpendicular. This high catching seemed to be a sub-conscious accommodation to the position for throwing. The balls, of course, glided down his hand before he could seize them and he made practically no advance until he held his hand lower with the palm turned upward. This improvement, which necessitated the further change of keeping the balls at a distance from his body, was consciously adopted on the thirty-third day and at that time a new rise began. The general flatness of C's curve is doubtless due to the fact that he never played ball when a boy.

F, on the other hand, on the third or fourth day found him-
self tossing the balls up at nearly arm's length to the right and
in such a way that they took a circular course and came down
in front of his body.   In this way collisions were avoided.
The plan entered upon unconsciously was then consciously
adopted, and as a result of finding a successful plan early in
the work his progress was rapid and his curve is the most
regular of the lot.   It may probably be regarded as typical
for muscular feats in which there is no long continued feeling
around for a successful way of doing the thing, as when the
learner is assisted by a good teacher.

B, again, tried pretty constantly, throughout, not to con-
sciously adopt any method, but to let everything take its
natural course and this seems to be the reason for his slow but
steady ascent without any high jump until near the end.   His
efforts in this direction did not, however, prevent his final ap-
proximation to a regular method though one less advantageous
than that developed by F.

We see in this the value of suggesting good ways of doing
things while the learning is still in its early stages.   If the
learner goes on he will finally develop a plan of his own but
only after a good deal of wandering and even then it may not
be the best.   But the suggestion to be effective should be
given at the time when need for it is keenest, at the "psycho-
logical moment."   It is then that its value will be felt.[1]

In polo, golf, baseball or football good form is absolutely
necessary for reaching a high degree of skill.   It is the essential
prerequisite for good methods.   Movement and position become
associated and a change in the latter requires relearning the
former.   The physiological limit of bad form is low.

In learning any complicated performance, we progress by
sections.   That is to say, the co-operating movements improve
separately.   This leaves certain errors conspicuous when we
are well along in the work.   Indeed the whole learning pro-
cess, at least in learning to toss and catch balls, seems to con-
sist in eliminating errors.   First the obvious ones are gotten
rid of, then new ones appear, and it is only after all have been
overcome that the thing can be regarded as mastered.

In avoiding errors there was adaptation, apparently more
organic than conscious, to conditions and often it was so deli-
cate as to elude observation.   B, for example, found himself
tossing high in order to have time to recover from a difficult
situation, and at another time he caught himself putting his
body into a more alert position by slightly raising himself on

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[1] Miss Shinn reports that her neice until six years old always ran
flat-footed, but when she was shown the advantage of keeping her
heel slightly lifted, she readily adopted it.

his toes and making his muscles tense. Then he realized that
he had been doing it for several days. So far as he could de-
termine consciousness had no originating part in it.

It is interesting that all the subjects improved by hitting
upon better ways of working without any further conscious
selection, at first, than the general effort to succeed. There
seems to be a competition of methods. Just how this selection
occurs without conscious interference is not easy to say. Con-
sciousness discovers modes of action already in use and selects
some of them for survival because of their success. They then
pass into the automatic. In this way reflex movements may
have first been conscious.

Two learned to throw in less than half the time that the
others needed, but their movements always called for a great
outlay of energy. Economy of effort is an important element
in effectiveness, but its getting requires time for the solidifica-
tion of associations and for the elimination of useless move-
ments, with the subsequent automatization of just those that
are essential to the process.

A certain amount of "warming up" was usually necessary.
While high throws were not confined to one part of the day's
trial, they rarely came at the beginning. Commonly, so long
as the score was low enough to eliminate the effect of fatigue,
the one or two high throws after the warming up period were
followed by a slump which again yielded to high ones toward
the end. This form of the daily curve was too common to be
entirely a matter of chance. It is another case of the uncon-
trollable variation of the maximum effort.

In his experiments on Practice and Habit, Johnson (14),
also, noticed that his subjects could not get control of their
muscles within the time of the preliminary tests.

Bryan and Harter have told us that it is intense effort that
counts, and this is true, but with a qualification. Throughout
this investigation it was clear that attempts to spurt were not
effective. Indeed, the very effort interfered with success by
making the attention too obtrusive. Special strain is itself a
distraction, as Johnson found it at times. It is steady and
calm intensity that counts for progress.

The importance of strenuous effort lies in the fact that up to
a certain point of intensity it is generally *successful* effort and
that is what counts, as Woodworth (25) and Johnson (14)
found.

Fatigue from any cause not only brings a lowering of the
day's score but the entire process of learning is probably hin-
dered. The growth of the nervous system into the required
forms of activity has been disturbed.

D felt that he had caught the knack when sickness in his

family brought loss of sleep, and it was nearly a week before the feeling of confidence returned.   Meanwhile his score dropped, except for occasional spurts, far below that which we should expect from his previous and subsequent record or from that of others.

F, too, felt that he was delaying his left hand progress by practicing when fatigued from lectures, and a change of hour brought immediate results.

In tossing and catching the ball a pretty general co-operation of all the muscles of the body is required, though those, of course, that effect the movements of the arm and hand are most directly involved.   Of these the movements most prominent in consciousness are the general movements of the arm.   The body movements, in most cases, do not come into consciousness at all, and the finer movements of the fingers and hands, except so far as they are covered by the general intention to toss and catch the ball successfully, are almost equally unregarded. As a matter of fact, however, skill in throwing and catching is rather more an affair of the fingers than of any other members.

The question then arises how are the necessary co-ordinations brought about?   It does not seem difficult to bring the matter into line with phenomena already pretty well known.   Let us suppose a successful toss and catch is made.   This is followed by a double effect; it leaves, as every action does, a trace in the nervous system which facilitates later repetition of the same action, and the successful adaptation also gives rise to a feeling of pleasure.   The effect of pleasurable sensation is a heightening of muscular tonicity or a general tendency to motor discharge, which in the case of an action just performed— one whose neural effects are still lingering—is equivalent to a partial reinnervation of the same co-ordinated group of muscles which again deepens the existing trace.   The next actual effort finds the nervous mechanism a little readier to react in this favorable way.   In case of an effort that does not lead to success, the slight displeasure at failure exerts its natural depressant effect upon the whole neuro-muscular system, and this does not deepen the neural trace left by the original movement, and even, perhaps, breaks up the incipient co-ordinations that gave it its particular form.   In any case, whatever its mode of action, it has not the reinvigorating effect upon the original neural trace exercised by the pleasurable sensation, and therefore, in the long run, the successful movements, and the co-ordinations upon which they depend, tend to persist while those that are unsuccessful tend to fall away.

Now it will be observed that this action of pleasant and unpleasant sensation does not depend at all on consciousness of the detail of the movements and applies as well to a movement

of which all the ultimate factors are unconscious and only the general end known. In such a movement, if the result is unsuccessful, a slightly different movement follows at the next trial, that is, one in which the co-ordination among the muscles engaged is slightly altered, as an automatic result of the partial inhibition of ill success. This new trial may be no better than the previous one, in which case it is again altered until success is reached or the attempt given up completely. In the case of movements the details of which come more or less completely into consciousness the same process goes on but with more rapid progress toward the desired end, because the variations from which the advantageous movements are selected are not chance variations, but are from the start more or less perfectly suited to the requirements of the case. In the ball tossing the general arm movements remain the prominent thing in consciousness, as we have said, while the finger movements are little noticed or quite neglected, and yet, nevertheless, the whole co-ordinated group is worked over, under the influence of the voluntary movements, into proper adaptation for the successful performance of the feat.

2. *The Effect of Right Hand Training upon the Skill of the Left Hand.* As already described (section above) the subjects in this investigation were all tested with their left hand before the right-hand practice began, in order that the effect of the latter upon the former for the left-hand tests, which stand at the right side of the charts given above, might be determined. In the curves the short straight line projecting to the right of the vertical axis shows the score made by the left hand in this preliminary test. The progress of the left hand in its subsequent practice is shown upon the same scale and in the same way as that of the right hand by the light and heavy lines of the diagrams.

Several things are at once noticeable.

1. The record of the first day of regular left hand training is in all cases higher than the preliminary test, though in no case had the left hand been practiced with the balls at all during the interval. More than this, the score never drops to the level of this preliminary test, which shows that the gain was permanent.

2. The left hand curves bear a striking resemblance in general form to the corresponding right hand ones with this difference that in all but one case they ascend much more rapidly. A did in eight days with his left hand what his right hand needed thirty-eight days to accomplish. E made a left hand record in four days that he had not been able to do in less than eleven days with his right. The difference with the others is not so marked, but in all cases but one the left hand curve rises more rapidly than the right.

3. All of the subjects but one made a better score with their left hand on the first day of its regular practice than they had been able to do with their right at the beginning of the work.

4. The highest and lowest left hand single throws are in almost all instances higher than corresponding right hand throws on corresponding days.

F was delayed in his left hand progress at about the middle of the work by physical and mental exhaustion. The anomalous record of B will be considered a little later in another connection.

The conclusion is unavoidable that in the majority of cases the training of the right hand was somehow effective upon the left also. The same general result has been noticed by many observers engaged in different lines of investigation, and has been made the subject of a special investigation by Davis.[1] The chief point of interest is to discover how the effect is produced. Is it due to some purely peripheral change, or to some alteration in the central nervous system, or finally to some method or plan of work that can be applied equally well in the case of either hand, as for example the knowledge of spelling which a man could use as well in writing in mirror script as in the ordinary way? It is not impossible that cases could be found that would exhibit the co-operation of all three. In the ball tossing there was evidence, certainly, of the last two. All the subjects were able to make use with the left hand of the methods of handling the balls, and of recovering control of them after an ill-directed throw, that had been developed in the right hand practice. In all the cases but one a good deal of a less conscious facility (of a sort that might indicate some kind of symmetrical training of the central nervous system) was probably present. In the case of B, whose record supplies the anomalous case in the left hand training, the "method" of tossing and managing the balls was distinctly carried over, but the less conscious neural habits were apparently not present. He could get out of difficulties with remarkable dexterity, but he failed in the simple things. He could not use these "higher habits" to the best advantage because he did not have the lower, and in his case these came with more than usual difficulty. The others were able at once to build in the sub-structure of central (or neuro-muscular) skill and so to learn the art of left hand throwing much quicker than the right. The mental element, the power to comprehend and meet a situation, is evidently, then, in most cases, the more difficult part of complex muscular feats of skill, since the right hand, if taken first, needs so much more time for the learning than the left,

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[1]See 9 (bibliography) also 10, 11, 18, 20, 21, 22, 23 and 24.

notwithstanding its greater general facility in such movements in right handed people.

The anomalous nature of B's left hand curve led to the inquiry as to whether he was relatively less skillful with his left hand than the other subjects. For testing this a target approximately seven feet six inches in diameter, with nine concentric circles each about four and a half inches wide, was used. The target had previously been used by A. W. Trettien. The test consisted in throwing one hundred balls with each hand from a distance of thirty feet. The bulls-eye was nine inches in diameter and the balls used were such as have been described above in the section on ball-tossing. The following shows the success of the left hand compared with the right in percentages. C $=$ 45 per cent., B $=$ 50 per cent., F $=$ 66 per cent. while A and E each gave 72 per cent. It is evident from this that, with the exception of C, B was relatively less skillful with his left hand than the others. C did not continue long enough with his left hand in the regular work to enable us to say just how his curve would finally have compared with that of B. If now we consider the skill of the right hand, merely, estimating it by success in throwing at the target, we have the following order, beginning with the best. F, E, B, C and A. Again, arranging the subjects in the order of their left hand skill, leaving the right hand out of consideration, we have, beginning as before with the best, E, F, A, B and C. It is evident from this that left hand deficiency is, at any rate, one cause of the anomalous form of B's left hand curve.

To return, now, to the subject of left hand training, it would be a mistake to suppose that such experiments in cross-education give support to the doctrine of "formal education." There is no evidence to show that training has general value. Indeed it all argues strongly for the influence of content. Volkmann (22) found that six months of regular practice in distinguishing small visual distances in which his eyes gained remarkable power, had no effect whatever on his ability to perceive small tactual differences. The right hand has had a great variety of training that ought to bring it along rapidly in ball-tossing on the principle of formal training, but this investigation shows just the reverse. The right hand learns it very slowly but the special training that comes from doing it enables the left hand, awkward and stiff as it is, to get control of the situation in about one-third of the time required by the right. Skill in certain lines may be serviceable in other similar processes, but its value decreases as the difference between the kinds of work increases, and in many cases it is probably reduced to zero.[1]

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[1] For a full discussion of this matter, with experiments, see the work of Thorndike and Woodworth (26) and Bair (3).

3. *The Effect on Skill of the Intermission of Training.* To find out how rapidly a feat of muscular skill could be forgotten three subjects were tested with their right hand one month after they had finished with their left and the test was repeated every thirty days for five months. The curve is given in the chart by the dotted lines between those for the right and left hand.

Instead of being a forgetting curve, it turned out to be a new curve of learning though the subjects did not touch the balls during the intervals.

During the second and third month E rose steadily in skill, his average and highest throws greatly exceeding those made at any time during the regular practice, while in the fourth month his average exceeded his highest previous throw and in the fifth month he made the astonishing average record of 337 catches in ten trials. In this test his lowest score was 135 catches, and, with the exception of this and one of 196, none of his scores fell below 300 while three exceeded 400 catches. A began his rise the third month, and with him, too, the highest throws were far above those made during the regular work. C's score just about held its own with that of the regular practice. Evidently the mind not only grows to the modes in which it is exercised but this mental growth may continue, for a time at least, after the practice has ceased.

Prolonged practice in any muscular exercise brings increase in the size (or efficiency) of the muscles as well as increase of skill in their use. This portion of the ball-tossing study gives evidence of the relative independence of the two. The point at which fatigue came during the regular practice, varied somewhat, of course, with different subjects, but it was first felt after about forty or fifty throws. If the subject recovered from this, he felt comfortable until he approached ninety or one hundred, when fatigue came for the second time. If he pulled through this, he was all right for another fifty, at least so far as fatigue was concerned, and so periods of vigor and fatigue alternated about every fifty throws. But during the experiments in ''forgetting'' fatigue came on earlier and at times was so exhausting that the subject could not continue. But skill during this time, in which muscular endurance decreased so rapidly, suffered no decline. Indeed, in two of the three subjects it greatly increased during the thirty day omission of practice.

That fatigue prevented us from seeing the full increase of skill during these months in which practice was omitted is evident from the fact that E made his remarkable series of the fourth month when fatigue was much less noticeable than in the three preceding tests, because he had been playing tennis

daily during the previous week. Then, again, in the interval between the tests of the fourth and fifth months he played tennis quite regularly and his unprecedented score at the last of these trials, five months after the end of the regular practice, shows a further increase in skill during the intermission of training, notwithstanding his loss in muscular endurance previous to exercising on the tennis court.

That there should have been little loss is not very strange, for it is well known that feats of bodily skill, like swimming, dancing or bicycling, are not wholly lost by long periods of disuse, but we were not prepared for a positive gain in skill. It is interesting in this connection to cite Houdin's (13) experience who tells us that having in the past learned to handle four balls while reading he was still able to keep three in the air and read at the same time though he had "scarcely once touched the balls during the thirty years preceding." And in *L'Année psychologique* (5) Bourdon reports experiments showing that there was not only no loss in skill in mental processes after an interruption of the training for a period varying from twenty-eight to thirty-eight days and longer, but in most instances there was a positive gain during the intermission, while facility in certain mental processes was not wholly lost after seven or eight years' omission of practice.

SUMMARY OF RESULTS AND GENERAL INFERENCES.

1. The curve for learning a feat of muscular skill, so far as this study may be regarded as typical, is concave toward the vertical axis. (See p. 211.)

2. Progress is never steady but always by jumps, with not one or two but many intervening periods of delay. (See p. 211.)

3. Practice with one hand trains the other, as has already been observed by earlier investigators. (See pp. 218-220.)

4. The gain seems to be due in part to the possibility of the transference of many points of "method" and their application to the throwing of either hand and in part, also, to a more direct effect of training, probably upon symmetrical portions of the nervous system. (See pp. 219-220.)

5. Method was hit upon and improved by the subjects of these tests at first without conscious intent.

6. The general physical condition of the subject greatly influences his skill in ball-tossing as well as the effectiveness of his practice from the point of view of acquisition. (See curves G and D and p. 212.)

7. Maximum effort, in spite of conscientious intentions to the contrary, is a variable standard, and emotional factors of various sorts probably affect the score and by inference the learning process as well. (See pp. 213-214.)

PEDAGOGICAL HINTS AND SUGGESTIONS.

It would hardly be possible for one interested in education to carry through a study of this sort and not bring away a plentiful crop of pedagogical hints and suggestions, some, perhaps, mere analogues of little worth, but others that point the way to interesting lines of investigation.   Without attempting to defend or enlarge upon any of them the writer may set down the following.

1. Growth in muscular skill stimulates intellectual development in the final determination of the method.   We get into ways of doing things unconsciously, find ourselves doing them, in fact, but later those that survive do so for a reason.   This is important in the development of children through play.

2. The effect on the experimenter of watching the lowest and highest throws is interesting.   Teachers are apt to estimate a good pupil by his best achievements, forgetting his comparatively few failures, while a poor pupil is estimated by his poorest work.   The best work is impressed upon our mind in the one case and the poorest in the other.   It has the same psychological basis as the saying "it always rains when I do not take an umbrella."

3. Monotony.

It is a platitude, which nevertheless must continually be dinned into the deaf ears of schoolmasters, that children do not see things as adults do, and to them future benefit is at best a poor recompense for present misery.   If remote interests often count for little with adults they surely have even less force with children.

4. The effect of fatigue during the learning process has not received the attention that it deserves.   Most of our tests of fatigue on school children, so far as they relate to work, have been made in connection with what the child had learned long before and which had become automatic.

If, as Woodworth (25) found and as these experiments seem to show, it is not mere practice but successful practice that counts, then school work continued to the point of fatigue is disastrous.

5. The influence on their general progress of watching their own advance from day to day was undeniable in the case of my subjects and it suggests the question of introducing it into school work.   As it is now children are always tested by new demands or by comparison with companions who are going along with them.   On this account they often feel that they are making no advance, because their marks do not show any, but if they could see their own curve grow from day to day they would not only be interested in its variations but would also be convinced of their progress.

6. Children evidently cannot be expected to make even slow continuous progress. They must have time to catch up with themselves, so to speak. There is evidence all through this investigation that there are moments when we run ahead of our power and delays are necessary that associations and habits may have time to set.

7. Bourdon's experiments, as well as this investigation, raise the question of how many recitations per week in a given subject will be most advantageous for children. It is by no means certain that one each school day brings the best results. They should, of course, be frequent enough to prevent loss of interest but also far enough apart to give sub-conscious processes a chance. Special investigations are needed here.

8. Periods of retardation may represent the physiological or psychological limit for the method used.

9. The fact that attempts to spurt instead of making for progress delayed it by bringing into prominence psychical activities that serve the learner best while in the background, counts strongly against cramming.

10. Suggestion of a good way to do the thing saves time that would otherwise be lost, but the suggestions must be made when the learner feels the need for them.

II. ON LEARNING SHORT-HAND.

When short-hand was selected to illustrate the more strictly mental side of learning, it was thought that this might give a typical curve and that, in any case, it would furnish an interesting parallel to the research of Bryan and Harter on the learning of telegraphy already referred to. The practice and tests, however, seemed to show that each branch of knowledge has its own characteristics which so greatly affect the learning processs that we can speak of a typical curve of learning only in a very general way.

For this test but a single subject (the writer) was available.

An hour and a half each day was given to the study and practice of short-hand and the study was continued through something over ten weeks. During the first half the study time was spent in writing, but in the latter half it was about equally divided every day between practice in writing and in reading what had just been written.

The subject was tested daily, at first in writing only; later in both reading and writing. In the writing tests James' Talks to Teachers was used for dictation. There is no doubt that his rich vocabulary had an effect upon the curve. Had some author with a less extensive range been taken it is probable that a more rapid rise would have commenced at about the second third and the ascent then have proceeded with continu-

ally increasing rapidity till the limit was approached, when it would have passed over into the usual physiological plateau.

An assistant read a few words, usually a phrase, then waited until, by pronouncing the last word, the subject indicated that he was ready for more.

The subject wrote as fast as he could for a period of ten minutes and his score for the day was then measured by counting the words and also the lines and parts of a line in order to equalize, so far as possible, the effect of long and short words.

The tests in writing began February 18th and were taken each day until April 4th after which they were omitted on Sundays.   They closed April 30th.   The reading tests did not begin until March 8th and there were two omissions, March 10th and 27th.   In other respects they corresponded with the writing tests.

The method at first adopted in the reading tests was for the subject to write down the words in long hand as he read them. The length of the test period was, again, ten minutes but only ten seconds were allowed for a word.   If the subject did not indicate within that time that he had the word time was called and he was required to pass on to the next.[1]   At the end of the ten minutes his written work was compared with the text and the number of words and lines correctly read was recorded. When about two-thirds through, however, the limit by this method was reached on account of the time taken in writing. This point is indicated by the break in the curve.   On the eleven following days we alternated between the plan of having the subject write the words as before and that of having him read them aloud while the assistant followed in the text noting the mistakes.   After that the latter method was followed to the end.   The curve for translating without writing is shown in dotted lines in the diagram below.

The material for the reading test was that which the subject had himself written ten days or more before, and so it had the natural difficulties increased by poor and sometimes incorrect writing.   It was fully "cold" and only once or twice was the reader helped by memory and then only in one or two sentences by the general idea.   The Pernin system of short-hand was used and no attempt was made to abbreviate by phrasing, as that would have introduced a new element.

The upper (writing) curve is smoothed by threes as before (see foot note p. 210) but that for reading is smoothed only to

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[1]The time was not always called with perfect regularity.   An ordinary watch was used and the assistant found it difficult to carry the task through the full ten minute period without slips, but the average time was pretty fairly kept and the differences due to this cause are probably insignificant.

the break. From that point the curve is drawn from the records for each day.

CURVES FOR LEARNING SHORT-HAND.
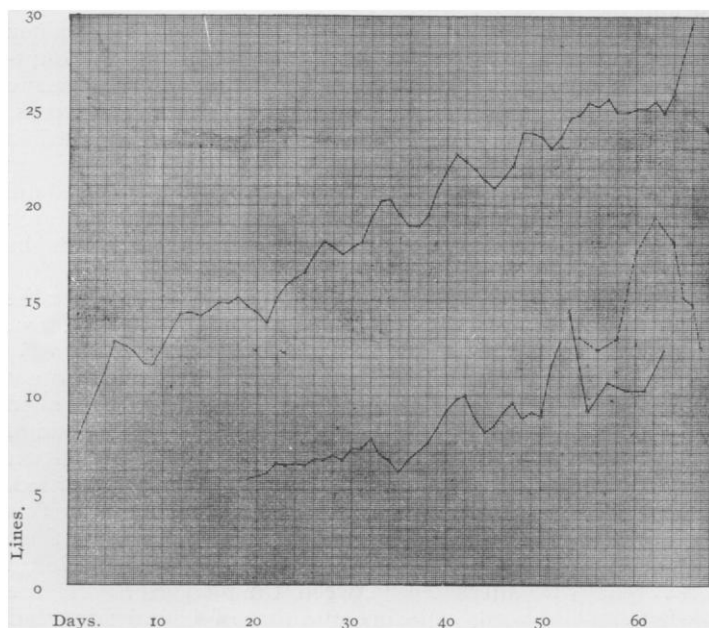
The curves for learning to write short-hand is above and that for reading the symbols below. The number and parts of lines written or read are shown on the vertical axis, the days on the horizontal. The break in the reading curve on the thirty-fourth day indicates the time when the subject began to alternate between writing the words in long hand as he read them, and reading them aloud while an assistant followed in the text noting the errors. From this point the curve gotten by the former method is given in the unbroken line as before, while the dotted line shows the progress when the score was kept by the latter method. (See p. 225.)

In the curve for writing (the upper one), the first rise from complete inability is rapid because the acquisition of a few imperfect co-ordinations and associations is easy and helps enormously when one starts at the zero point. Very soon, however, the subject makes all the gain possible of this rapid sort and then the rise settles down to a more or less gradual ascent. This curve would naturally outstrip that for reading at the start because the writer knows what he is to say and is not delayed by wrong translation of a preceding word, as in reading.   Relieved of these embarrassments the writer in short-hand

and, probably, the sender in telegraphy, can give his whole attention to getting off the word before him.

After the force of this initial rise is spent, retardation alternates with progress. The difference between this beginning rise and the periods of lesser gain that follow is that a little gain makes more showing at the start. There is no evidence furnished by this investigation of a special time for the formation of "higher habits," or association groups. When there is continued arrests in progress its sufficient cause, in the writer's opinion, is to be found in the emotional factors of learning. This will be further discussed below.

In ball-tossing the initial rise was practically absent, and in learning short-hand it lasted for only a few days.

Bryan and Harter (7), in the receiving curve for telegraphy, found a rapid initial rise followed by a period of little or no progress, or, as they term it a "plateau," and they conclude that "learning to receive the telegraphic language consists in acquiring a hierarchy of psychophysical habits" and that "a plateau in the curve means that the lower-order habits are approaching their maximum development, but are not yet sufficiently automatic to leave the attention free to attack the higher-order habits."

The nearest parallel in my work to telegraphic receiving is the reading of the short-hand notes. A glance at the chart will show that nothing of the initial rise is to be found in the curve for reading and that there is nothing also that looks like a "plateau." The short-hand writing is more nearly comparable with the telegraphic sending, the curve for which as given by Bryan and Harter[1] shows a rapid rise at the beginning, but no such rise is apparent in my writing curve except for the first few days while the symbols were being learned. (4.)

Of course the amount of daily practice in ball-tossing, as managed in my experiments, increased as skill was gained. The learner threw more balls before he missed. But this increase of practice with growing skill is characteristic of all learning. A beginner in short-hand, and in telegraphy also, gets in more practice to the hour the second week than the first. So the ball-tossing was not wholly exceptional in this respect. In order, however, to see the effect of equal amounts of practice, A's curve was replotted in such a way as to show the progress per thousand throws. Even this failed to show convexity.

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[1] None of the curves for learning to toss balls, as already noticed, have the initial rise with the succeeding "plateau" that characterize Bryan and Harter's curves.

The writer does not wish to dispute the gradual formation of such a hierarchy of habits as Bryan and Harter have described, nor to call the accuracy of their work in question, but he believes that the explanation of the order and the method of formation of such habits is different from that suggested by these authors and in particular that the "plateaus," when they occur, are to be accounted for in other ways. This immediate rapid rise at the beginning seems to be true only of these things that have symbols or other devices for handling and presenting ideas, and it is probable that after this first spurt, the length of which would vary with different sorts of material, the general form of the curve for learning is concave until the physiological limit is approached. Telegraphing involves fewer symbols, and the distraction of deciding on sounds and abbreviations, that mark the learning of the Pernin short-hand system, would not so greatly disturb the beginner, and so, having less thinking and deciding to do at the start, the learner in telegraphy would probably go on improving without great set backs longer than the short-hand writer.

In reading short-hand it was found that the speed rose with great rapidity whenever the reader was able to get the context. So long as he read by words, every word had to stand for itself, but when he got control of enough words to give him the sense it was no longer necessary to recognize each word. He could even correct in the reading those that were wrongly written.

The effect of getting the context was seen in occasional spurts soon after the reading tests began. Later they became more frequent.

The material for testing was taken as it chanced to come in the book, and the sudden drop in the reading curve at the end was due to its unusual difficulty.

In learning short-hand, and presumably also in learning to receive or send telegraphically, a large number of associations are formed that do not affect the speed of work, because there is no opportunity to use them and the learner seems to make little or no progress, not because this is the particular time for the formation of a "hierarchy of habits," for this is going on all the time, but because the range of association knowledge in the subject is too limited to meet the demand. After enough has been accumulated to meet the demands of the tests the curve will rise more rapidly. The associations that have been forming from the start have now become numerous enough to be effective.

The essential thing in getting a purchase here is to learn enough to make the associations already formed available. When this is done the rise comes.

That the failure of this material to influence the curve earlier is not altogether due to the lack of higher association-habits is evident from the fact that whenever the context is gotten, symbols that had long lain unused and were almost forgotten come in for service with the rest.

Associations are more active during the periods of delay only in the sense that there is more material to work upon than before, since they are not present until after the first rise that accompanies the rapid gathering of symbols at the start, but this is true of any later stage in the process when compared with an earlier one.

Bryan and Harter (6) tell us that in telegraphing "the learner enjoys the practice of sending, but feels practice in receiving to be painful and fatiguing drudgery." Unfortunately this feeling cannot be gotten rid of by setting aside certain periods for work. Practice is less effective and ennui may reach such a degree that little or nothing is accomplished during the hours of work. The effect of this monotony could be seen in the ball-tossing, but it was less influential there for reasons already given.[1] In the hours of practice upon the short-hand, however, its influence was great and wholly uncontrollable. The lack of content made the material so dry that the attention would wander in spite of every effort on the part of the subject to make his periods of practice equally valuable.

I can hardly doubt that this emotional factor was largely influential in making the plateau that Bryan and Harter found. Visible success is always interesting to the one who succeeds, but after the first dash has been made, and the easy acquisitions have been gathered in, the rest of the work involves a good deal of drudgery and all the indirect interests that may be brought into play cannot wholly relieve it of this drag. The beginner in German is fully conscious of the initial rise and thinks it great fun to say "*Guten morgen*" and "*Wie befinden Sie sich heute*" and his progress has certainly been great and rapid when compared with the zero with which he started, but the fun ends and the depressing monotony weigh him down long before he has reached the point where he can understand a native.

The wearying monotony and discouragement of this intervening period lessens the efficiency of the work which was so effective during the first spurt. Then, when the learner has reached the point where he can get the context and guess successfully, his practice increases without conscious effort and the pleasure of success causes him to redouble his efforts.

Psychical adaptation, too, greatly affects the curve and it is

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probably this almost unconquerable tendency to remain in the stage that just meets our needs, rather than the lack of higher association-habits, that makes it so difficult for an operator to rise above the skill necessary for his own office work and that prevents years of daily practice from bringing a man to his own maximum ability to receive. Johnson (14) also thinks that Bryan and Harter's plateaus are "resting places in the effort," and in a large measure this is probably true.

Summary. Some points of agreement in motor and mental learning may be set down here by way of summary.

1. As in ball-tossing the learner seems to make no advance for a time and then springs to a higher level, perhaps only to fall back a little but, at all events, not to go higher until he has strengthened his position here.

2. I have found no evidence for one or two special periods of delay in progress in which preparation is made for a higher order of habits. As before, automatization is going on throughout the process.

3. Here, too, consciousness discovers certain methods in operation and approves or disapproves of them. So improvement goes on through elimination and selection.

4. The effect of variations of maximum effort was apparent also in the tests with short-hand, and here the periods of discouragement did not always coincide with the 'plateaus.' Several times the subject was confident that he had beaten his record until the count proved that he was still on his old level.

5. Physical condition was a no less determining force than before. Many times the drop could be clearly traced to it.

6. As with the balls over strained attention was a hindrance. Signs that were gotten incidentally and used occasionally, without special effort to fix them, were remembered better than those upon which a good deal of effort was expended. Effort defeated its own end by calling into consciousness other similar signs.

Symbols just learned, and which could be used with considerable facility when the subject was alone, could not be recalled under the tension of the test. The very fact of being tested had an inhibiting influency on the mental processes.

The pedagogical bearing of these observations is obvious.

III. On the Origin and Control of the Reflex Wink.

The experiments on control were the first undertaken and it will be convenient to consider them here.

Darwin tells somewhere of standing before a cage of snakes with his face close to the glass and finding himself unable to resist the instinctive tendency to protect his eyes from harm by winking and jumping back when they struck against the glass,

and that, too, though fully conscious that the plate glass was thick enough to absolutely remove all danger.

Since the wink is a protective reflex of the greatest importance for the protection of sight in animals with movable eyelids, and so for their preservation, it is of no little interest to determine under what conditions it is amenable to the will. Some of these conditions had already been investigated by Partridge (17).

In the following experiments which were directed to a more detailed study of the means by which control of the wink is learned the subjects were, again, university students and one professor.

The apparatus consisted of a framed piece of plate-glass about six by eight inches in size which was attached to a steel rod supported on four legs. A small wooden-headed hammer was attached to the lower part of the frame behind the glass in such a way that when it was pulled down and released by the experimenter it would fly up and strike the plate glass. The wooden head of the hammer was covered with cloth and a strip of rubber so as to reduce the sound somewhat and lessen the danger of cracking the glass.[1]

In the first series of experiments the stimulus was both visual and auditory, no further effort being made to lessen the noise caused by the blow of the hammer or the rattle of the apparatus than that described above. The subject sat with his face close to the glass and his chin resting on a frame support. Effort was made to keep from winking but there was no strain of the attention or muscles. In order to determine whether the period of the day had any noticeable effect, tests were made in the morning before the subject began his work and again at noon and, finally, they were repeated late in the afternoon just at the close of the day's work. They were continued under these conditions for five days.

Results. The time of day and such fatigue as comes from an ordinary day's mental work had no appreciable effect. Three subjects tested this by reacting in the morning, before the day's work, at noon and finally late in the afternoon.

First day $\begin{cases} \text{1st Series of 20 trials—W} = 9, \text{ P} = \text{o, I} = \text{o.} \\ \text{2nd \quad '' \qquad '' \qquad '' \quad —W} = 14, \text{ P} = \text{o, I} = \text{o.} \end{cases}$

N. B. Throughout these experiments W = full wink, P = partial wink, *i. e.*, the least noticeable eye movement in response to the excitation and I = inhibition of the wink. After a little practice the experimenter was able to distinguish four degrees of winking, depending upon the force of the movement and completeness of the process. It

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[1]The apparatus was at first the same as used by Partridge, but as the experiments progressed it was altered from time to time in minor details.

has seemed best, however, to consider only the full wink and the least noticeable reaction to the stimulus.

Fifth day $\begin{cases} \text{1st Series of 20 trials—W} = 2, \text{ P} = 7, \text{ I} = 3. \\ \text{2nd } \text{''} \text{''} \text{''} \text{—W} = 1, \text{ P} = 12, \text{ I} = 3. \end{cases}$

A series of artificial devices for distracting the attention was then tried with the same subject. Some of these diversions were winking fast between the strokes of the hammer, biting the lips and refraining from swallowing the saliva until it became annoying. These distractions were very effective when first adopted but soon lost their power. The following are samples of reactions made on different days under the influence of varying distractions. The entire series occupied seven days. Distractions of this sort were rarely effective longer than two days.

        20 trials—W $= 0$, P $= 17$, I $= 2$.
        20 trials—W $= 0$, P $= 5$, I $= 12$.
        20 trials—W $= 0$, P $= 3$, I $= 17$.
        20 trials—W $= 0$, P $= 3$, I $= 17$.
        20 trials—W $= 1$, P $= 7$, I $= 12$.

At this point a new subject was taken and he was instructed to put his attention on inhibiting the wink but to avoid any muscular strain. As there are great individual differences it does not seem best to compare the reactions of different subjects but rather to follow the attempts at inhibition of each subject under varying conditions.

As before, the following are the reactions for the first day. The tests were made in the morning, at noon, and late in the afternoon.

  1.   20 trials—W $= 7$, P $= 3$, I $= 0$.
  2.   20 trials—W $= 1$, P $= 15$, I $= 1$.
  3.   20 trials—W $= 5$, P $= 11$, I $= 0$.
  4.   20 trials—W $= 0$, P $= 10$, I $= 4$.
  5.   20 trials—W $= 7$, P $= 4$, I $= 3$.
  6.   20 trials—W $= 1$, P $= 10$, I $= 8$.

Six days' practice brought no improvement. The result on the last day was

  1.   20 trials—W $= 4$, P $= 2$, I $= 2$.
  2.   20 trials—W $= 0$, P $= 9$, I $= 5$.

The same subject now tried the effect of focusing his attention on a point on the wall behind the hammer and directly in its path. In another series he thought of some just discernible dots on the wall at the same time keeping his eyes fixed upon them. The purpose was to withdraw the attention from the excitation. The improvement over the reactions when the attention was on inhibiting was marked. These experiments, which occupied four days and included two hundred and

eighty excitations in series of twenty each, caused a total of only fourteen full winks. The following are samples taken from the first and last days.

1. 20 trials—W = 5, P = 12, I = 2.
2. 20 trials—W = 0, P = 7, I = 13.
3. 20 trials—W = 0, P = 13, I = 5.
4. 20 trials—W = 3, P = 6, I = 5.
5. 20 trials—W = 1, P = 19, I = 0.

It may be mentioned here that very little improvement was observed so long as effort was directed merely to inhibition. Control seemed to come in proportion as excitation and inhibition were forgotten.

A series lasting four days, in which the attention was given to the thought of keeping his attention on a black target behind the hammer, gave practically the same results as the series that we have just considered.

The effect of reducing the sound by closing his ears with his index fingers was now tried. Series with the ears open were alternated, but with some irregularity, with those in which they were closed. Six days were given to these and the following show the results of the first and last. The attention in both sets was again on the thought of keeping his attention on the black target behind the hammer.

First day, ears open:

1. 10 trials—W = 8, P = 0, I = 0.
2. 10 trials—W = 4, P = 1, I = 0.
3. 10 trials—W = 7, P = 0, I = 0.
4. 10 trials—W = 5, P = 1, I = 0.
5. 10 trials—W = 6, P = 1, I = 0.
6. 10 trials—W = 3, P = 0, I = 0.

First day, ears closed:

1. 10 trials—W = 0, P = 0, I = 4.
2. 10 trials—W = 1, P = 6, I = 3.
3. 10 trials—W = 0, P = 3, I = 7.
4. 10 trials—W = 0, P = 0, I = 10.
5. 10 trials—W = 3, P = 6, I = 1.
6. 10 trials—W = 3, P = 3, I = 1.

Last day, ears open:

1. 20 trials—W = 4, P = 2, I = 0.
2. 20 trials—W = 5, P = 2, I = 0.
3. 20 trials—W = 1, P = 5, I = 1.

Last day, ears closed:

1. 20 trials—W = 0, P = 1, I = 18.
2. 20 trials—W = 1, P = 2, I = 15.
3. 20 trials—W = 0, P = 6, I = 12.

It is evident that the sound of the hammer striking against

the glass has been an important element in the preceding reactions.

At this point in the experiments the head rest was removed and during the rest of the investigation the subject sat free but with his face close to the glass as before.

The conditions of the series just described were now altered by the introduction of another element. The subject made his muscles tense leaning slightly forward in his chair and grasping his knees firmly with his hands. The result, however, did not differ greatly from the preceding. The reactions for the last day were—

Ears open:
1.   20 trials—W = o, P = 8, I = 9.
2.   20 trials—W = o, P = 8, I = 11.
3.   20 trials—W = 1, P = 12, I = 1.

Ears closed:
1.   20 trials—W = 3, P = o, I = 15.
2.   20 trials—W = o, P = 1, I = 18.

The subject had a feeling during this series that the muscles of the arms and legs were too far removed from the eyes to make their contraction effective in inhibiting the wink and so this gave way to contraction of the facial muscles and those of the neck but the reactions continued about the same.

The contraction was then extended to the muscles of the head and scalp. The effort was so great as to throw the face into a quiver of distortion. At the same time a control series was introduced in which effort was directed merely to inhibiting, but without any unusual muscular tension. The following are the reactions for the last of five days' practice.

Ears open:
1.   20 trials—W = o, P = 3, I = 17.
2.   20 trials—W = o, P = 1, I = 19.
3.   20 trials—W = o, P = 2, I = 18.

Ears closed:
1.   20 trials—W = o, P = 2, I = 17.
2.   20 trials—W = o, P = 3, I = 17.

Three "control" series taken on the last three days gave
1.   20 trials—W = o, P = 18, I = o.
2.   20 trials—W = o, P = 8, I = 10.
3.   20 trials—W = 3, P = 1, I = o.

It is evident from these results that contraction of the muscles of the scalp together with the other muscles of the head greatly increased the power to inhibit. This means of control was not only the most effective but it also retained its influence longer than the others. In time, however, it, too, lost its hold but as its effect lessened, the general power to control

without the aid of any muscular contraction seemed to increase. As this improvement had not been observed while the practice was confined to attempts to inhibit directly, it seemed worth while to try to determine how much time would be needed for learning to control this reflex through adjacent muscles, and the experiments directed toward this will be discussed later.

A series in which contraction of the face and scalp muscles was alternated with some in which the hands were tightly clinched clearly showed the greater effectiveness of the former in facilitating control, and finally the effect of slightly bending the head forward without any intentional muscular contraction was tried. This gave a feeling of security on account of the over lapping eye brows and the position necessarily occasioned some contraction which doubtless aided in the control. The result was much the same as when the face and scalp muscles were contracted.

In order to test the effect of distraction of the attention without muscular contraction, a series was taken in which the subject added columns of figures on a card behind the hammer. These were alternated with others in which there was only effort to inhibit. In order that the effect of previous practice in inhibition through muscular contraction might be eliminated a new subject was taken for this series. The results failed to show any effect of this kind of distraction on the wink reaction. The experiments were continued for twenty days.

Having found that we may learn to control the reflex wink by beginning with muscle situated near the eyes and gradually getting hold, as it were, of the eye muscles themselves, the question suggested itself as to whether this control could be gotten in a short time or whether the long and tedious training that our subject went through is necessary. Eight subjects in all were tested. Of these two succeeded after the first or second excitation and repeated trials failed to bring any noticeable reaction afterward.[1] Two were unable to learn to inhibit within a reasonable time, i. e., two or three weeks, and the progress of the others is shown below.

The instructions were necessarily rather general. They were to contract the facial muscles and then to try by elimination to get control of just such muscles as proved effective in the inhibition.

Subject A: First series, Dec. 5.
      20 trials—W = 4, P = 5, I = 3.
Subject A: Final series, Dec. 13.
      20 trials—W = 0, P = 4, I = 16.
Subject B: First series, Dec. 4.
      20 trials—W = 1, P = 15. I = 2.

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[1] *Cf.* also the experiments of Partridge (17.)

Subject B: Final series, Dec. 9.
        20 trials—W = 0, P = 1, I = 18.
Subject C: First series, Jan. 7.
        20 trials—W = 1, P = 12, I = .7.
Subject C: Final series, Jan. 11.
        20 trials—W = 0, P = 0, I = 20.
Subject D: First series, Dec. 30.
        20 trials—W = 6, P = 5, I = 0.
Subject D: Final series, Jan. 3.
        20 trials—W = 1, P = 5, I = 14.

The evidence of those who succeeded was that they began with mass muscular control and gradually worked down to the essential ones by "getting their feeling." The time required was about a week. Two succeeded in five days. At the last test there was no muscular contraction noticeable to the experimenter and the subjects themselves felt no more eye tension, as they said, than would be necessary to observe a fine point at a little distance.

That this control is not due to distraction of the attention was shown by a further experiment with D immediately after his final test. He read aloud rapidly while the hammer struck against the glass as before, holding his book in such a position as to bring the hammer within the margin of the field of vision. The result was

        20 trials—W = 3, P = 8, I = 0.

We may then summarize the results of this part of the investigation as follows:

1. The eye reflex is a complex reaction resulting from a combination of visual and auditory sensations and the final effect seems to be greater than the sum of the separate effects. Probably the order in which the stimuli are experienced has something to do with this.

2. Moderate fatigue does not noticeably affect the reaction.

3. Attention on inhibition only slightly reduces the number and intensity of the reactions.

4. Attention on distracting objects gives moderate and temporary results.

5. Distraction of the attention by various devices lessens the reaction but they soon wear out.

6. Adding has no appreciable effect.

7. Contraction of muscles remote from the eyes does not greatly reduce the reactions.

8. Contraction of muscles near the eyes has decided effect.

9. Closing the ears markedly lessens the reaction.

10. The training in control gotten through contraction of the muscles near the eye seems to have an effect on the simple

inhibitory control (*i. e.*, that in which the attention is on inhibiting), increasing its efficiency up to a certain point beyond which a great deal of practice does not carry it.

While the experiments that we have just described were in progress one of the subjects put his twenty-five weeks old baby-girl before the apparatus, the effect of which he had been trying for weeks to learn to resist but with only partial success, and to the surprise of the observers her eyes were wholly unaffected though her face was close to the plate glass, against which the hammer was striking with so much noise that most adults could learn to restrain the wink only after considerable practice, and she was looking directly across the hammer's course. This occurred at the end of the university year, and fourteen weeks later, when the close of the vacation permitted another test, the child winked every time the hammer struck. After two or three blows she looked frightened and turned to her mother, as though about to cry. During the previous test she had been undisturbed mentally as well as reflexively.

These observations suggested enlarging the investigation to include the genesis of the reflex wink, and through the kindness of two sets of parents in the university circle the writer was able to make careful tests of the matter upon two young babies.

For the first series of experiments a baby-girl was the subject. The tests were made during the middle of the afternoon. The conditions of sleep and health were noted when necessary. Tests were begun on her 68th day. On that day I made forty rapid passes with my index finger toward her eyes, almost touching them, but she did not wink. Neither did she when her eye lashes were touched with a handkerchief but if the conjunctiva was touched she usually winked, though not always, and the winks were chiefly confined to a "fresh eye," *i. e.*, she soon became accustomed to it and ceased to respond. This could hardly have been a matter of fatigue of the sense organ or nerves since care was taken to avoid this and the failure to respond was noticed after two or three winks.

Before making the tests with the apparatus we passed a brass spring about the size of the hammer back and forth in front of her, at a distance of two feet from her face, and found that she followed it with her eyes in whatever direction we held it.

Care was taken throughout the investigation to guard against fatigue and, with a baby, it is not difficult to determine when this condition is being approached.

Two sets of experiments, each of twenty trials, were then made with the hammer. In these the auditory and visual excitations were combined, as in the case of the adults. The baby was held so that her face was close to the glass with her

eyes on a level with the hammer as it struck against the other side. The force of the blow was sufficient to give one quite an unpleasant shock.

In the first set of twenty tests she winked five times and in the second three. But they were not merely winks. The baby started or jumped slightly and the wink was part of this general reaction. This will be found to play an important rôle in the development of this reflex.

On her 75th day the experiment was varied so that we might observe the effect of sudden visual excitation. A piece of soft cloth was folded several times and held between the handle of the hammer and the body of the apparatus so as largely to eliminate the sound of the hammer's blow. This rather primitive method was adopted because after trying several more elaborate plans it was found the best. The problem was to have the hammer fly up with its full force directly against the glass but to strike it so lightly as not to have the sound a disturbing element. To do this it was necessary to stop it suddenly, just before it struck the glass, and the cloth padding did this fairly well. There was usually a little noise but apparently not enough to be a disturbance.

As before, the baby's face was brought close to the plate glass with her eyes on a level with the hammer as it flew toward her. Whenever her eyes wandered from the direction of the hammer the experimenter waited until he could get her attention again or, sometimes, her father walked about with her so as to avoid fatigue and keep conditions as natural as possible.

In the first twenty tests there were two winks but they seemed entirely natural and not caused by the hammer. In order that there might be no mistake about this, one hundred and fifteen additional tests were made in sets of about twenty each, and in no instance was there the slightest trace of a wink except such as occurred between the intervals of the stimulation, and care was taken that the hammer strokes should not follow one another so rapidly as to cause any confusion here.

The experiment of touching her eye lashes with the corner of a handkerchief was repeated and she winked in about seventy-five per cent. of the cases.

On her 82nd day we began three sorts of tests all of which were made each time during the remainder of the investigation. The auditory and visual combined and the visual alone have just been described. In addition we tried the effect of sound alone. In this the baby was held so that her eyes were turned away from the apparatus while one ear was toward the glass and near it.

For convenience I will designate these three sorts of tests as "auditory—visual," "visual," and "auditory."

The visual tests were always made first so as to avoid any possible nervous effect of the noise from the hammer.

82nd day. I. Visual. (Sound largely eliminated by padding.)
1. 25 tests . . . . 3 winks.
2. 25 " . . . . 0 "
3. 25 " . . . . 6 "

While some of these winks seemed natural, this was not true of them all, and it was evident that she was gaining in this reflex.

II. Auditory.
1. 25 tests . . . . 6 winks.

Several of these seemed natural, but with one or two of them the start, which at first characterized every response, was noticeable. Her general reaction, however, had greatly decreased.

III. Auditory—visual.
1. 20 tests . . . 11 winks.

She looked frightened and at times seemed on the point of crying, quite in contrast to her behavior two weeks before, on her 68th day, when her response was more organic than selective.

89th day. She had slept very little during the day and was nervous and fretful both before and during the tests. This explains the irregularity of the results. They are interesting in showing her reaction during sickness but should not be regarded as showing her natural reflex response at this time.

I. Visual.
1. 20 trials . . . 3 winks, but only one of these seemed to be caused by the excitation. The other two had every appearance of being natural.
2. 20 trials . . . 6 winks.
3. 20 trials . . . 0 winks. During this series she was looking directly across the path of the hammer at a little girl in a red dress who was standing at the lower end of the table. Her attention being taken up in this way probably prevented her from winking, notwithstanding the other favorable conditions.

II. Auditory.
1. 20 trials . . . 17 winks.
2. 20 trials . . . 8 winks.

In the second series the first five winks began with the second excitation and followed one another in succession. After that she skipped one, then winked, while the other came toward the last.

III.   Auditory—visual.
    1.   20 trials .   .   .   15 winks.
Toward the last she seemed bored but not frightened.

96th day.   I.   Visual.
    1.   20 trials .   .   .   5 winks and two of these were surely
natural.
    2.   20 trials .   .   .   5 winks and one was apparently nat-
ural.
    3.   20 trials .   .   .   2 winks but both were evidently nat-
ural.
    4.   20 trials .   .   .   13 winks.   Only one of these seemed
natural but she was looking straight at the hammer and made
wink after wink, consecutively, while looking at it.   The ex-
perimenter took care not to have the stimuli follow one another
so fast as to have a summation of effect.
II.   Auditory.
    1.   20 trials .   .   .   6 winks.   Four of these appeared
to be natural.
III.   Auditory—visual.
    1.   20 trials .   .   .   7 winks.
    2.   20 trials .   .   .   6 winks but only two seemed to be
caused by the apparatus.

103d day.   I.   Visual.
    1.   20 trials .   .   .   2 winks.
    2.   20 trials .   .   .   4 winks.
    3.   20 trials .   .   .   1 wink.
    4.   20 trials .   .   .   1 wink.

So far as we could judge all of these were responses to the
excitation.

In order to see the effect of a larger object we then held her
behind a glass door, with her face toward the glass, and struck
the opposite side with a black cap.
    1.   20 trials .   .   .   1 wink.
    2.   20 trials .   .   .   1 wink.
II.   Auditory.
    1.   20 trials .   .   .   8 winks.

Here a variation was introduced.   A piece of card board
was tacked to the apparatus in front of the hammer so that she
could be held facing it, as in the visual series, but could not
see the hammer.
    1.   20 trials .   .   .   17 winks.
    2.   20 trials .   .   .   13 winks.

We concluded, however, that the increased number of winks
was an effect of the louder and sharper noise that the card
caused the hammer to make rather than the result of facing
the apparatus.

III.   Auditory—visual.
   1.   20 trials   .   .   .   14 winks.
   2.   20 trials   .   .   .   11 winks.
    Most of the winks in the second series were toward the beginning.
    Whenever her attention was taken by discomfort caused by her position she did not wink

110th day.
    The tests to-day were not altogether satisfactory because the baby was so restless that it was hard to keep her attention. She was not irritable but was more than usually taken up with herself and the objects around her. She seemed more interested in the experimenter than in the experiments.

I.   Visual.
   1.   20 trials   .   .   .   10 winks.
    Two of these came late and one was undoubtedly natural.
   2.   20 trials   .   .   .   9 winks.
   3.   20 trials   .   .   .   9 winks.

II.   Auditory:
   1.   20 trials   .   .   .   3 winks.
    All of these came with the start that has characterized all these reactions at the beginning.
    We repeated the variation, introduced on her 103d day, of putting a piece of card board in front of the hammer so that she could face the apparatus.
   1.   20 trials   .   .   .   9 winks.
    Three of these were only half winks but they seemed to be responses to the noise.
   2.   20 trials   .   .   .   6 winks.
    Four of these were very slow.

III.   Auditory—visual.
   1.   20 trials   .   .   .   5 winks.
    Sucking her fingers seemed to take her attention during this series.
   2.   20 trials   .   .   .   7 winks.

117th day.   I.   Visual.
   1.   20 trials   .   .   .   17 winks.
   2.   20 trials   .   .   .   12 winks.
   3.   20 trials   .   .   .   11 winks.
    During the latter part of the third set it was evident that her attention was taken up by something beyond the hammer, and as a result she did not wink.
   4.   20 trials   .   .   .   10 winks.
    Here, again, she seemed to be looking at some object.

II.   Auditory.
   1.   20 trials   .   .   .   11 winks.

In addition to these there were four partial winks.

2. 20 trials . . . 4 winks.

During the first part of this series her attention was taken up with trying to do something, and she did not begin to wink until she found that she could not accomplish it and so released her attention.

3. 20 trials . . . 8 winks.

Besides these there were four partial ones. Her attention was again occupied during a part of the time and then she did not wink.

The start which characterized her reaction to auditory stimuli at the beginning has been occurring less frequently and to-day was not noticeable

With the piece of card board in front of the hammer and the baby again facing it we found

1. 20 trials . . . 18 winks.

2. 20 trials . . . 14 winks.

In the second set there were also three partial winks.

III. Auditory—visual.

1. 20 trials . . . 18 winks.

She also moved her eye lids, as though about to react, the remaining two times but did not finish the wink.

2. 20 trials . . . 16 winks.

In addition to these there were four partial winks.

126th day. I. Visual.

1. 20 trials . . . 11 winks.

2. 20 trials . . . 14 winks.

3. 20 trials . . . 13 winks.

Delayed winks were quite noticeable in these series.

II. Auditory.

1. 20 trials . . . 5 winks.

2. 20 trials . . . 7 winks.

The card board variation, again, gave the following.

1. 20 trials . . . 9 winks.

There were also two or three quivers.

2. 20 trials . . . 8 winks.

Besides these, two quivers were noticed but during this series the card board attached to the apparatus seemed to take her attention.

3. 20 trials . . . 4 winks.

III. Auditory—visual.

1. 20 trials . . . 12 winks.

2. 20 trials . . . 14 winks.

In addition to these there were four partial winks. The other two times not a muscle moved.

3. 20 trials . . . 13 or 14 winks.

The uncertainty here arose because we were unable to determine whether one was natural or not.

In an investigation of this sort there is much that figures cannot show. The general organic reaction must be a matter of observation, and I find in my note for this day, "she is evidently becoming less affected by auditory stimuli while her reaction to visual excitations is continually increasing." In reference to this her parents said, also, that she had lately begun to notice everything in the room.

131st day.   I.   Visual.
    1.   20 trials   .   .   .   16 winks.
In addition there was one partial wink.
    2.   20 trials   .   .   .   15 winks.

An organic response, somewhat similar to that noticed at an earlier age in connection with the auditory excitations, was observed at about this time in the visual reactions. She sometimes winked with great force, shutting her eyes together tightly, as though the visual excitation disturbed her. This sort of a response to auditory stimuli had long since entirely disappeared.

II.   Auditory.
    1.   20 trials   .   .   .   3 winks.
    2.   20 trials   .   .   .   7 winks.
    3.   20 trials   .   .   .   5 winks.
With the card board in front of the hammer.
    1.   20 trials   .   .   .   3 winks.
    2.   20 trials   .   .   .   5 winks.

III.   Auditory—visual.
    1.   20 trials   .   .   .   8 winks.
Several times in this series, when she was looking directly at the hammer, she winked fiercely.
    2.   20 trials   .   .   .   13 winks.
    3.   20 trials   .   .   .   12 winks.

In the last two series she winked whenever she was looking right across the hammer's path and, apparently, at nothing in particular.

141st day.   I.   Visual.
    1.   20 trials   .   .   .   10 winks.
    2.   20 trials   .   .   .   11 winks.
    3.   20 trials   .   .   .   10 winks.
II.   Auditory.
    1.   20 trials   .   .   .   3 winks.
    2.   20 trials   .   .   .   0 winks.
    3.   20 trials   .   .   .   2 or 3 winks.
    4.   20 trials   .   .   .   0 winks.

III.   Auditory—visual.
   1.   20 trials   .   .   .   20 winks.
   2.   20 trials   .   .   .   20 winks.
   3.   20 trials   .   .   .   19 winks.

The organic response reached its culmination in the auditory—visual to-day. In this series she frequently shrank back and looked frightened, closing her eyes and keeping them closed for several seconds as though the stimulus pained her.

152nd day.   I. Visual.
   1.   20 trials   .   .   .   10 winks.
   2.   20 trials   .   .   .   13 winks.

She seemed somewhat more sensitive than usual in this series.
   3.   20 trials   .   .   .   6 or 7 winks.
   4.   20 trials   .   .   .   5 winks.
   5.   20 trials   .   .   .   5 winks.

II. Auditory.
   1.   20 trials   .   .   .   7 winks.
   2.   20 trials   .   .   .   7 winks.
   3.   20 trials   .   .   .   0 winks.
   4.   20 trials   .   .   .   0 winks.
   5.   20 trials   .   .   .   0 winks.

III. Auditory—visual.
   1.   20 trials   .   .   .   19 winks.
   2.   20 trials   .   .   .   20 winks.
   3.   20 trials   .   .   .   18 winks.
   4.   20 trials   .   .   .   19 winks.

The second subject studied was a baby boy. In his case the investigation was begun at a little earlier stage.

As before, a careful test was made to be sure that he could see an object the size of the hammer at a considerably greater distance than that at which the hammer would approach him.

46th day.   I. Visual.
   1.   20 trials   .   .   .   0 winks.
   2.   20 trials   .   .   .   0 winks.
   3.   20 trials   .   .   .   0 winks.
   4.   20 trials   .   .   .   0 winks.
   5.   20 trials   .   .   .   0 winks.

II. Auditory.
   1.   20 trials   .   .   .   20 winks.
   2.   20 trials   .   .   .   20 winks.
   3.   20 trials   .   .   .   20 winks.
   4.   20 trials   .   .   .   20 winks.
   5.   20 trials   .   .   .   20 winks.

I have called his reactions to the auditory stimulus winks

but that does not characterize them properly.   At every stroke of the hammer he shrank back, much as a small boy does when one makes rapid passes at his face with the hand.   And then, in addition, he shut his eyes tightly as though the sound were painful.   Still he did not jump, at least not perceptibly. The latter mode of reacting seems to be a later stage of the organic response.   (*Cf.* also p. 244.)

It was noticed that after twenty excitations, he sometimes seemed to become accustomed to the stimulus and ceased to react.

53d day.   I. Visual.
  1.   20 trials   .   .   .   0 winks.
  2.   20 trials   .   .   .   0 winks.
II.   Auditory.
  1.   20 trials   .   .   .   15 or 16 winks.
  2.   20 trials   .   .   .   17 winks.

The same shrinking in response to the auditory stimuli was observed only it was a little less marked.   The so-called winks are not yet that but rather the participation of the eyes in the general organic shrinking reaction.

60th day.   I. Visual.
  1.   20 trials   .   .   .   0 winks.
  2.   20 trials   .   .   .   0 winks.

These series were unusually successful in the fact that he looked directly through the glass across the field of the hammer all the time.   For this reason delays, efforts to get his attention, were unnecessary.

II.   Auditory.
  1.   20 trials   .   .   .   6 winks.
  2.   20 trials   .   .   .   8 winks.

The shrinking, hitherto so noticeable, was hardly apparent to-day.   He paid little attention to the sound except for the winking reaction which now for the first time took the place of the shrinking.   This was also the first day when he did not act as if the stimulus pained him or as though he wanted to cry.

67th day.   I. Visual.
  1.   20 trials   .   .   .   0 winks.
  2.   20 trials   .   .   .   0 winks.
II.   Auditory.
  1.   20 trials   .   .   .   9 winks.
  2.   20 trials   .   .   .   8 winks.

One or two of the winks in the last series may have been natural.

The shrinking reaction was entirely absent and there was no evidence whatever that the sound disturbed him; indeed, to-

day seemed to mark a turning point in his psychic life, since, for the first time, he turned his head to see what was making all the noise.

74th day. I. Visual.
    1. 20 trials . . . 0 winks.

One occurred but it seemed perfectly natural.
    2. 20 trials . . . 0 winks.

II. Auditory.
    1. 20 trials . . . 15 winks.
    2. 20 trials . . . 3 winks.

In the last series his brow contracted seventeen times but in only in the three cases did it go on to the wink. There were thus seventeen reactions to the excitation but they were of varying degree. This may indicate a transitional stage.
    3. 20 trials . . . 19 winks.

83d day. I. Visual.
    1. 20 trials . . . 0 winks.
    2. 20 trials . . . 2 winks.
    3. 20 trials . . . 2 winks.

II. Auditory.
    1. 20 trials . . . 17 winks.
    2. 20 trials . . . 15 winks.

87th day. I. Visual.
    1. 20 trials . . . 5 winks.

Three of these were certainly caused by the apparatus, the other two were uncertain.
    2. 20 trials . . . 4 winks.

One was accompanied by the characteristic reflex "start" which occurred now for the first time in the visual reactions. (See p. 241, *cf.* also pp. 244 and 245.)
    3. 20 trials . . . 2 winks.
    4. 20 trials . . . 5 winks.

One or two in the last series seemed natural.
    5. 20 trials . . . 5 winks.

II. Auditory.
    1. 20 trials . . . 8 winks.
    2. 20 trials . . . 6 winks.

On his 71st day his father found that he winked occasionally when he thrust his finger at his eyes but to-day he reacts every time to this stimulus.

It is interesting, in connection with the development of the reflex wink, to learn that on his 86th day, just when the visual reflex was making its most rapid development, the child suddenly burst into a violent fit of crying and hid his face in his mother's lap at the sight of a neighbor's baby.

Unfortunately the experiments were brought to an end at

this time by the baby being taken out of town and it was not possible to test him further until his 130th day.

130th day. I. Visual.

    1. 20 trials . . . 5 winks.

In addition to these there were two or three partial reactions, *i. e.*, slight contraction of the brow with noticeable movement of the eyes.

    2. 20 trials . . . 12 winks.

II. Auditory.

    1. 20 trials . . . 15 winks.

    2. 20 trials . . . 13 winks.

I am indebted to Superintendent Harold Barnes for the following note regarding another of his boys. At the end of the baby's eighth week his father noticed that he did not wink however suddenly an object might be brought close to his eyes.[1]

Miss Shinn (19) tells us, too, that her niece winked reflexively for the first time on her fifty-sixth day, when a head was suddenly thrust close to her face.

The following general conclusions may be drawn from this investigation.

    1. Until about the fiftieth day babies are excessively sensitive to auditory stimuli.

    2. The first response to a sudden sharp sound is organic. It is a general bodily contraction, which may be accompanied by a jump, but if not, it always develops soon into this mode of response, and in this general reaction the eyes participate. The experiments on the baby-girl, the first subject, were evidently begun at about the close of this period. Later, as this investigation shows (and in the case of the little boy, the second subject, it was about the sixtieth day), the bodily response ceases and the wink becomes differentiated as a distinct reaction.

    3. The visual reflex does not appear until much later. In the two cases investigated it was first observed shortly after the eightieth day.

    4. Sensitiveness to auditory and visual stimuli pursue an inverse development. As the auditory reaction becomes less marked the visual appears and gradually increases in frequency until the adult condition is approached, in which the visual response is common and the auditory rare.

    5. Reflex reaction, so far as the wink may be regarded as

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[1]Preyer (16) found that his child winked at the quick approach of an object to his face on his sixtieth day. He also noticed the slower closing of the lids than in adults but speaks of it only up to the twelfth day whereas in this investigation it was observed at times in the baby-girl up to her one hundred and twenty-sixth day.

typical, is evidently inherited in the race but learned by the
individual.   The elements from which the reactions can be
built up are given in heredity, but the nicety of their adapta-
tion to ends must be learned.

The manner in which the first subject reacted to the audi-
tory—visual stimulus on her 82nd day and the greater number
of winks usually caused by this double excitation is interesting.
Can it be that at this early day sight of an approaching object
gives new significance to the noise that immediately follows?
Her attitude toward it on that day indicated a fear wholly
absent in the auditory reactions that just preceded.

The auditory and visual tests were also made on another
baby-boy on his 265th day.   As the earlier experiments had
indicated that withdrawal of the attention from the excitation
greatly alters the reaction, two sets of visual tests were taken
in alternate series, one such as we have already described while
the other differed in having a lighted match held behind the
hammer so that, as the baby looked across its path, his atten-
tion would be taken by the light.   The results are given in the
order in which the experiments were made.

265th day. I. Visual.
　　20 trials . . . 14 winks.
　　Visual with lighted match.
　　11 trials . . . 3 winks.
　　The first seemed to be a natural wink.
　　Visual.
　　19 trials . . . 19 winks.
　　Visual with lighted match.
　　20 trials . . . 6 winks.
　　Two of these seemed caused by the sudden appearance of
the light.
　　Visual.
　　20 trials . . . 10 winks.
　　Visual with lighted match.
　　18 trials . . . 3 winks.
　　One was so gentle that it seemed natural.
　　In the series with the lighted match the winks came late in
the series, usually at the last.
II.   Auditory.
　　20 trials . . . 0 winks.

These experiments were repeated on his 296th day but the
match did not hold his attention as before.   Whenever his
eyes rested on it, however, or on the experimenter, who stood
behind the hammer, the reactions were less frequent than at
other times.

In order to find out the part played by sound and sight in

the reflex wink of older persons, experiments were made on four university students and on a five year old boy.

In order that the sound might be reduced as much as possible the ears of the subjects were packed with cotton which was kept in place by a bandage passing under the chin. This was

TABLE.

	A				B				C				D					E			
	C	V	A	X	C	V	A	X	C	V	A	X	C	V	A	V¹	X	C	V	A	X
W	6	0	0	0	7	0	0	2	2	0	0	0	20	0	17	0	0	17	0	4	4
W¹	4	1	0	0	7	0	0	0	4	0	0	0	0	1	3	0	0	2	1	1	2
W²	5	4	0	1	2	1	0	0	10	0	1	6	0	0	0	0	3	1	0	1	3
W³	5	5	6	9	4	0	2	2	4	0	2	4	0	8	0	1	2	0	1	7	2
W⁴	0	7	4	5	0	0	8	0	0	0	6	1	0	0	0	0	0	0	0	4	0
O	0	3	10	5	0	19	10	16	0	20	11	9	0	11	0	19	15	0	18	3	9

C = control, i. e., auditory and visual with no attempt to isolate any element in either.
V = visual.
A = auditory.
X = auditory with eyes turned toward the apparatus but hammer covered.
W = full wink, W¹, W², W³, W⁴ = lesser degrees of winking, W⁴ indicating the slightest noticeable twitch of the eyes.
O = no wink.
All of the subjects were university students except "E," who was a boy of five and a half years.
V¹ in "D" gives the results of a further attempt to eliminate sound by having an assistant press the bandages close to the ears of the subject.

in addition to the cloth bumper that broke the force of the hammer as it was about to strike the glass.

The sound could not be entirely eliminated, but this arrangement so muffled it as largely, if not wholly, to neutralize its influence on the reactions.

It is evident from the table that the reaction caused by excitation of the wink reflex, when visual and auditory elements are combined, is the result of both factors, and the part played by each varies with different individuals. In "A" the auditory stimulation was less effective, with "B" they did not differ greatly while in the others the reactions to the auditory was more marked.

Partridge (17), with a similar apparatus, in a series of experiments on children from five to fifteen years of age, found a gradual improvement in control with increasing age, and these results show that Dr. Sanford[1] was right in ascribing much of the effect to the sound of the hammer and to the rattle of the apparatus. Further, the tests on the five and a half year old boy, 'E,' do not indicate any essential difference in the reactions after that age.

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[1] See bibliography 17 foot note.

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